

On the "Law of Fatigue" regulating Muscular Exertion*

II.

WITH regard to Mr. Nipher's new series of experiments published in NATURE (vol. xi., p. 276), in Table II., I shall make only two observations:—

1. That they appear to me to be subjected to too much of reduction and discussion, a process which does not always improve experiments, and that the intervention of an assistant who lifts the weight from the experimenter appears to introduce new sources of error, both as regards the work done and the punctual observance of time of lift.

2. That the formula (cubical hyperbola) objected to by Mr. Nipher, which is derived from the "Law of Fatigue," represents his new series of experiments quite as well as the complicated empirical formula which he has employed, and which has no theoretical meaning.

I simply give the following table, taken from Mr. Nipher's Table II., and calculated from the formula—

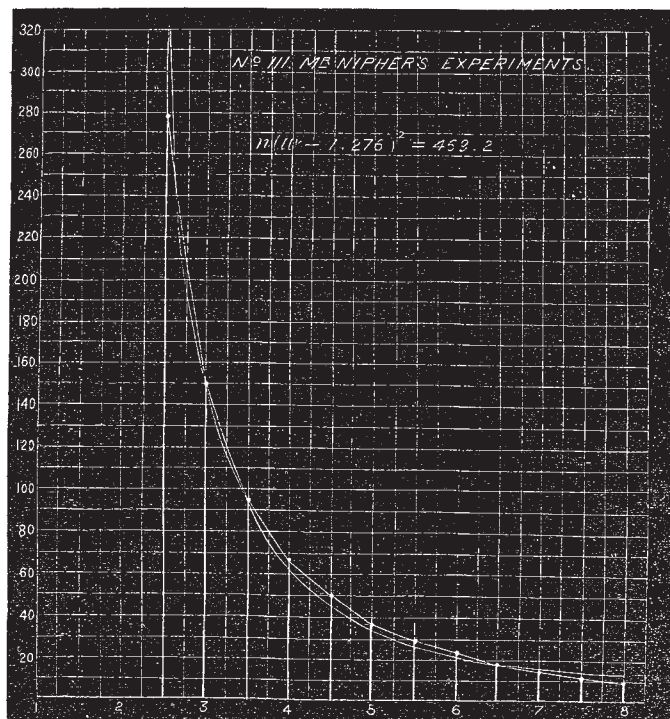
$$n(w + a)^3 = A$$

where $A = 469.2$, $a = -1.276$.

Comparison of Nipher and Haughton's Calculations with Nipher's Observations.

w	n (obs.)	n (calc.) Nipher.	n (calc.) Haughton.	Diff. Nipher.	Diff. Haughton.
2½	283	242	313	+ 41	— 30
3	152.5	150.3	157	+ 2.2	— 4.5
3½	95.8	99.4	94.7	— 3.6	+ 1.1
4	67.2	69.2	63.3	— 2.0	+ 3.9
4½	51.2	50.1	41.5	+ 1.1	+ 6.1
5	36.9	37.4	33.8	— 0.5	+ 3.1
5½	28.6	28.7	26.2	— 0.1	+ 2.4
6	22.7	22.5	21.0	+ 0.2	+ 1.7
6½	18.1	18.0	17.2	+ 0.1	+ 0.9
7	14.5	14.6	14.3	— 0.1	+ 0.2
7½	10.4	11.9	12.1	— 1.5	— 1.7
8	7.7	9.9	10.4	— 2.2	— 2.7

I also give a diagram (No. 3), showing to the eye the agree-



ment between the shape of the cubical hyperbola and Mr. Nipher's observations. In fact it is plain, either from the table or the diagram, that my formula, derived from the "Law of Fatigue," represents his observations fully as well as his own empirical formula.

In Mr. Nipher's former experiments already given, the value of a comes out to be + 1.094, and in the present experiments it is a negative quantity and equal to - 1.276; whereas, according to direct observation, it ought to be + 1.50. According to the Law of Fatigue, the value of a should be half the weight of the arm, and a negative value of a is absurd.

The absurdity, however, is easily explained, and is not in the "Law of Fatigue." The "Law of Fatigue" asserts that the total work done by a group of muscles tired fairly out is inversely proportional to the rate at which they are condemned to act; but it tacitly supposes that the group of muscles in question is not aided by other muscles in any way. This is very difficult to prevent, and it can only be accomplished by a careful study of the muscles used, and by devising a rigorous posture and movement during

* Continued from p. 466.

the experiments, such as shall compel the group of muscles to do their work and prevent other groups from helping them, which they endeavour to do, from the strong animal instinct of avoiding pain. In the experiments made by myself, Dr. Macalister, and Mr. Gilbert Haughton, the muscles used were two—

1. Supraspinatus
2. Deltoideus Acromialis:

and the palms were supinated, and the plane of motion was the transverse plane.

In Mr. Nipher's experiments (if I understand his description correctly) the plane of motion was 45° in advance of the transverse plane, and the hand was probably pronated. These circumstances would allow the muscles already named to be aided in an irregular manner by the following muscles:—

3. Deltoideus clavicularis
4. Trapezius scapularis (anterior fibres)
5. Pectoralis major (superior fibres).

The assistance supposed given to the group of muscles which are tired out is not sufficient to fatigue the muscular fibres called into

play irregularly, and the Law of Fatigue will not apply to them; and the statement of that law leading to the cubical hyperbola must be modified as follows:—

Let there be m fibres tired out,

And n fibres worked but not tired out;

And let x be the mean weight held in the hand lifted by the fibres n ; then the weight really lifted by the fibres m will be $(w + a - x)$. And it is to this quantity only that the Law of Fatigue applies, giving us the formula

$$n(w + a - x)^2 = A. \quad (4)$$

In Mr. Nipher's first set of experiments at fixed rate we found—

$$a - x = + 1.094.$$

And in his experiments now published we have—

$$a - x = - 1.276.$$

From this (supposing the experiments not damaged in their reduction) I should infer that the supraspinatus and acromial deltoid were aided, irregularly, in the two cases by muscular fibres (not tired out), which lifted, respectively 0.41 and 2.77 kilos.

Trinity College, Dublin,
March 13

SAMUEL HAUGHTON

P.S.—I have received a letter from Prof. Gustavus Hinrichs, of Iowa State University, in whose laboratory Mr. Nipher was assistant, and who gave Mr. Nipher all possible aid in his experiments. In this letter Prof. Hinrichs states that Mr. Nipher's former experiments were in fact as good as those he last made. I myself believe that, in some respects, they were better.

Denudation

MANY students of geology find a difficulty in realising that the effects of denudation are due to the simple action of water set in motion only in ways familiar to us. To them, and indeed to many others, it may be of some interest to observe a working model which, though made without any such design, shows with curious fidelity, on a small scale, the effects which have been produced in the lapse of ages on the great features of our globe.

Londoners will remember that the Serpentine was emptied, cleaned out, and finally refilled about five years ago. Coping-stones of hewn granite were laid along the margin of the foot-path, and from this, slanting down for about two feet, was a layer of concrete laid about the level of the water line. Possibly this concrete was not of the most durable quality, still it was certainly harder than most of the rocks which bound our coasts. But in the short space of about five years the tiny wavelets of this little lake have worked this *smooth sloping hill* into a bold and rugged line. In some places, indeed, all the concrete has been washed away, and there is a sandy beach right up to the granite. Two or three years ago the water was at a somewhat lower level than it is now. The traces of the change are recorded, especially on the north side, a little to the east of the boat-houses. There, a double range of "cliffs," one over the other, is to be seen extending for some considerable distance.

This "model" is indeed of so much interest that I ask you to insert this notice of it, for I am sure that many of the readers of NATURE would share the pleasure I have felt in watching the very striking similarity in effects produced by the same agents working on scales so vastly different.

R. H.

OUR ASTRONOMICAL COLUMN

THE SUN'S PARALLAX.—In *Astron. Nach.*, No. 2,033, Prof. Galle, Director of the Observatory of Breslau, gives his final deductions with reference to the value of solar parallax from corresponding observations of the minor planet Flora, about the opposition of 1873, which took place while the planet was near perihelion. Observations with this special object in view were made at the Observatories of Bothkamp (Herr von Bulow), Cape of Good Hope, Clinton (N.Y.), Cordoba, Dublin, Leipsic, Lund, Melbourne, Moscow, Parsonstown (the Earl of Rosse), Washington, and Upsala; by 37 N. and 36 S. stars, the sun's parallax is inferred to be $8''.879 (\pm 0''.0396)$, which, singularly enough, is the exact figure lately communicated by M. d'Abbadie to the Astronomer Royal, as a first result obtained by M. Puiseux, from observations of the

recent Transit of Venus at the French stations at Pekin and St. Paul Island.

TUTTLE'S VARIABLE NEBULA IN DRACO, &c.—This object well deserves regular observation, the evidence in favour of its variability being apparently beyond question. It was first seen by Tuttle in September 1859, and occurs in Argelander's *Durchmusterung*. On the 24th of September, 1862, D'Arrest, observing with the Copenhagen refractor, describes it as a large bright nebula, $2'$ long and $80''$ broad, and he adds: "bene conspicienda tubo quæstore." On the 22nd of August, 1863, after re-examination, he has the note: "I think this nebula was far brighter in the year 1862," and on the 12th of the following month he remarks: "tubo quæstore non amplius discernitur." In a letter to Sir John Herschel, he expresses his conviction that the nebula could not have been so bright as it was in September 1862, in the time of Sir W. Herschel and Messier. Auwers, in *Königsberg Observations*, xxxiv. p. 227, says he found the nebula pretty bright, $2\frac{1}{2}'$ long, $1\frac{1}{2}'$ broad, the direction of the longer diameter being 50° . If we take the mean of D'Arrest's observations for position (*Siderum Nebulosorum*, &c. p. 333), and bring up to the commencement of 1875, the following place results:—

R.A. ... 18h. 23m. 16s. N.P.D. ... $15^\circ 29'5$

This nebula is No. 4,415 of Sir John Herschel's general Catalogue. We are able to state that there is some suspicion of variability about No. 4,369 of the same Catalogue (Hind, 1852, April 26), and possibly in the small hazy-looking star preceding the brightest part of the nebula. In April 1852 it was very small and rather faint, perhaps $1'$ in diameter; it followed Lalande, 33076, $50''$ s., and was 9.4 north of the star. Auwers (*Königsberg Observations*, xxxiv. p. 227) found it pretty faint, $2'$ diameter, gradually a little brighter towards the middle; a star 12th magnitude situate on the border of the nebula on an angle of about 230° from its centre. Later observations have afforded indication of fluctuating brightness, but are not decisive. Auwers thought he found signs of variability in the nebula No. 4,473 (Hind, 1845, March 30). In a 6-feet Fraunhofer it was pretty bright, round, and from two to three minutes in diameter; and once, 1860, Aug. 16, with the Königsberg heliometer it was "surprisingly faint and of the second class at the highest." Schönfeld has several observations in *Astronomische Beob. zu Mannheim*, 1862; the diameter is variously recorded between $45''$ and $2'$, and once it is remarked that the nebula showed strong scintillation and appeared resolvable. D'Arrest, who independently discovered this nebula in the spring of 1852 (*Astron. Nach.*, No. 809) has given his earlier observations in *Resultate aus Beob. der Nebelflecken, Erste Reihe*; in September 1855 he suspected it might prove a cluster of very minute stars. His later observations with the Copenhagen refractor are published in *Siderum Nebulosorum*, &c., where he states that he had not, during sixteen years, noticed any change either of brightness or position; and he mentions further that in April 1866 he detected a number of luminous points. Variability in the case of this object appears hardly to rest upon sufficient proof, considering the effect of indifferent nights upon such observations, but it is suggested in Sir John Herschel's last Catalogue, and on that account is referred to here.

COMET 1766 (II.)—If Burckhardt's elliptical elements of the second comet of 1766, discovered at Paris on April 8, are approximately correct, it is not improbable that the comet was observed on its first perihelion passage with that form of orbit. Burckhardt succeeded in representing the rough observations of La Nux at the Isle of Bourbon, extending to May 13, by an ellipse with a period of only five years, Pingré having failed in bringing them into satisfactory agreement with the few observations taken by Messier and Cassini de Thury, at Paris—